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THE INVESTIGATION OF OCEAN PASTURAGE

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When one speaks of pasturage, the term usually brings to mind a vision of tender grasses and lush herbage, of waving fields and verdant hillsides, of well-kept flocks and contented herds. If one has a question or a problem concerning this pasturage, he has but to step out upon the field or plain, make a sweeping survey with his eye, and decide on place and plan for getting the answer to his question or for approaching the solution to his problem.

Not so with the pasturage of the sea (the phytoplankton), invisible to the unaided eye. Here we can envisage but a waste of waters, its surface unbroken except by wind or wave or by the struggle of some predaceous animal or its prey. Though a drop of water with its myriads of microscopic plants may include a pasturage for scores of tiny animals, we can obtain from it but little information as to pasturage conditions throughout the ocean.

As a general problem that looms limitless before our vision, we find ourselves confronted by an area of incomprehensible extent, a volume of water beyond the human imagination, and a fluid medium of very limited accessibility. As a consequence, no plan of attack can be arranged without long and arduous preliminary studies done at great expense, with expert assistance and complicated equipment, and often at great risk, hardship and heavy loss. Nor can these adverse factors be removed from consideration in a permanent program of investigation.

Clearly, then, the study of ocean pasturage must always remain largely in the hands of a devoted few. The experienced herdsman may look hastily over a pasture land and satisfactorily answer his own question as to how many cattle it will support for a given number of days, or as to how much danger there is in the presence of a certain poisonous weed. The experienced fisherman can do nothing of that sort. He must have the qualified expert to tell him whether the ocean pasturage in March was sufficient to support the young fish then getting a start, or whether it was so scant that he may expect a slack season in which over-fishing may be fatal to his own interests.

It must not be understood from this casual reference to the qualified expert that such a service is yet established. Even in the much investigated North Sea area of Europe the records are still too imperfect for that. In spite of the enlistment of some of the greatest scientific men of the world, progress has been slow and results fragmentary. How, then, is provision to be made for advancing scientific inquiry into the conditions of existence of marine pasturage, and for making effective use of available resources in future investigation? Since European scientists have tried every conceivable method of work, various schemes of cooperative effort, and numerous plans of attack on oceanic problems, there is little hope of innovation. The best prospects seem to lie in the development and continuance of methods, activities and plans already known and tried.

The studies which are of most general importance are ecological studies, and they are also the studies most seriously hampered by natural conditions just mentioned. In the first place, we must find some solution for the problem of collecting material. Tow nets of very finely meshed mill silk are most extensively used for this purpose, yet there is no possible way of determining the amount of water that goes through such a net while being towed. This is because that amount is affected by form, age, and previous usage of the net, by number size, form and adhesiveness of organisms, by temperature, salinity, pressure and viscosity of the water, by direction, speed and continuity of currents in the water, by rate, direction and method of hauling the net, and by the motion of the boat. Evidently, then, tow net catches give no trustworthy indication of the relative amounts of phytoplankton in two different locations or in the same location at two different times, although they may have a high suggestive value.

If, however, we simply dip up a given quantity of water (say, twenty-five liters) and pour it through the net, we at least know approximately how much water is handled. While it is still true that temperature, viscosity and many other factors cause marked differences in the numbers of organisms lost through the pores of the net at one time and another, these differences are not so great as to forbid comparison, and they can be more readily compensated by repeated series of catches. Evidently this is the method which may most reasonably be taken as a standard either for isolated catches or for extended and continuous work. Further points in its favor are the simplicity of equipment, the low cost of the smaller net, the rarity of its loss or replacement, the lack of demand for technical skill in handling it, and the possibility of using it under widely varying conditions. This is well shown in our recent experience at the Scripps Institution, for which I have been running a daily series of collections from our pier, while a pier employee near Oxnard, California, is running a similar series, our business manager has made three off-shore series from a pleasure yacht, our physicist has secured a series from a coastwise passenger steamer, and an officer on the same steamer is running a somewhat broken series. By what other method is such diversity and continuity possible while yielding comparable results?

The catches just mentioned have all been surface catches. When one goes below the surface difficulties increase. However, this same method is available, the only difference being in the difficulties of handling the closing bucket or other means of getting water from a given depth.

Our comment has not yet disclosed all of the variables (or sources of error) incident to collecting oceanic organisms. Errors in sampling are always present and usually very large. Ordinarily we have no means of knowing whether a catch represents a fairly uniform distribution at the particular depth over a wide area, or whether it is taken from the midst or the edge or the outside of a swarm when distribution over the area is very irregular. Nor can we know any more as to distribution in the vertical planes at the locality sampled. Such errors in sampling can only be met by unlimited increase in the number of samples taken and by the greatest practicable continuity in taking them, continuity in time, continuity in space.

By such unlimited increase in number and in practicable continuity carried over periods of decades or centuries we may arrive at gradual cancellation of the multitudinous errors and reach a viewpoint of substantial accuracy. Such has been the history and development of astronomy, meteorology, tidal predictions and various other phases of physical and mathematical sciences. We certainly can not know how nearly certain phases of biological science may be brought to the effectiveness of generalization characteristic of those other sciences until we have accumulated records similar in extent and continuity to the past records upon which their present effectiveness is based. A prime essential in the accumulation of numerous and continuous records is simplicity and adaptability of method. So far as collecting material is concerned, the dipping method seems to most nearly meet those requirements. The adoption of such a method for a standard does not in any sense forbid the use of tow net, centrifuge and cultural methods for supplementary work, or for special series where conditions are favorable. In fact, any other methods which can possibly be used for check and interpretation of results of the standard method are always to be welcomed.

After having secured material, the question of its preservation arises. On account of ease of handling, the use of formalin is to be recommended as a standard practice. In those cases in which experts are able to look after the collections and to command facilities for desired manipulation special preservatives may be used or the material may be studied in the living condition. Here, again, we must say that the easiest method that is generally usable should be the standard, and that methods of greater refinement should be supplementary or subsidiary.

Material once collected is, of course, usable for various purposes, such as taxonomy, morphology, variation studies and ecology. Inasmuch as the ecology of marine microplankton organisms must be largely (and often entirely) studied through some type of statistical examination, it surely seems most desirable to give first consideration to the demands of that study. Material used for other purposes may have its statistical value destroyed. Statistical handling, however, does not affect its value for other purposes. If, then, quantitative work is to be done with any given material, it ought to be done before that material is used in other ways.

After having secured and preserved material by standard methods, there are still some difficult questions to settle as to details of quantitative study. Some observers take one cubic centimeter of the material at a certain dilution and count all of the diatoms and dinoflagellates in this quantity as it is spread out in a Sedgwick-Rafter counting cell. This method requires that tally be kept up into the thousands of organisms in many cases, and that several hours shall be spent in the tedious enumeration of individuals on the one slide. Not only does this method introduce the error of excessive fatigue into the work, but it also uses an amount of time which prohibits the examination of any large number of samples. Its questionable gain in accuracy is not at all commensurate with its drain on time and energy.

The law of diminishing returns deserves just as careful consideration in scientific enterprises as in any other line of human activities. If so much time is spent in examining a few samples that we must neglect many others, there is distinct loss in our work when dealing with problems so wide and deep and intricate as these of marine pasturage. Even if absolute accuracy of collecting, preserving and counting of a given sample were possible, it would be of little value until the result could be compared with those of hundreds or thousands of similar samples. Hundreds of samples approximately enumerated will give a much better idea of actual conditions in the sea than ten samples enumerated with excruciating care. Hence it is necessary to reduce the labor of counting to the lowest point at which we can reach approximations which are justly comparable.

As a matter of fact, the mixing of the material so as to distribute organisms through the fluid has a very important bearing on the accuracy of the count. After some experiment, I have adopted the standard plan of shaking the bottle of preserved material for one minute and then reversing twenty times each mixing tube used in diluting the catch. I then count one eighth of the slide. By this uniform treatment of all catches I think I am able to keep the error of enumeration well within plus or minus twenty-five percent in most catches, according to indications from some tests which I have made.

If I should again undertake work on fresh-water microplankton, I think I should use these same methods for standard methods and follow the same general plan of operation. If arrangements could be made by which workers on microplankton everywhere should adopt these or similarly adaptable methods for standard, it would greatly facilitate comparison of results obtained in different localities under widely varying conditions. It seems to me that some such provision ought to be made in connection with all work of this kind, in order to hasten the accumulation of records from which we may eventually derive indices to the determining factors in production and distribution of phytoplankton.

In summarizing let me say that I believe the investigation of marine phytoplankton to be of fundamental importance not only because these synthetic plants constitute the sustaining pasturage for oceanic animals, but also because the conditions of their existence are such that they are most likely to furnish dependable indices of the relative importance of the various factors which influence vital existence. That I believe it necessary for proper advancement in their investigation to adopt standard stations for collection, and also standard methods of collection, preservation and examination, the prime requisite of which shall be continuity of the series. That I believe simplicity to be essential and statistical approximations much more valuable than statistical refinements. And, lastly, that I believe in using every possible special method for supplement and check on the standard methods.